XVII.—*A Reply to Mr. H. G. Seeley's Remarks on my Account of the Phosphatic Deposit at Potton, in Bedfordshire.* By J. F. Walker, B.A., F.C.P.S., F.C.S., F.G.S., Sidney Sussex College, Cambridge.

In April 1866 the Rev. P. B. Brodie wrote a paper on the phosphatic deposit near Potton, in Bedfordshire, and stated that the fossils were derived from preexisting formations*. Having obtained from this bed some additional fossils, especially remains of *Iguanodon*, I wrote a short paper, supplementary to Mr. Brodie's, which was published in the Number of this Magazine for July 1866. At this period the Woodwardian Museum contained no fossils from this deposit; but since then, through the exertions of Mr. Keeping, who has the care of the Museum, it has obtained a fine series of these fossils. In August of the same year Mr. Seeley published a letter criticising the results arrived at by Mr. Brodie and myself; but this fact does not appear from his reference to that paper in the last Number of the 'Annals,' in which he would seem to intend to represent himself as the person attacked, instead of the aggressor, in this matter. Mr. Seeley stated in his letter that all the fossils appeared to him to be "denizens of the old sea-bed where they abound;" and this is the chief point on which our views do not coincide. Mr. Seeley says that the only mistake in his paper is the statement that "the *Gryphæa dilatata* is perversely wanting." But I am not surprised that Mr. Seeley obtained no specimens of this fossil, as the work-people did not save the ferruginous shells until I told them to do so†. I will now consider Mr. Seeley's criticisms *seriatim*.

I. Mr. Seeley objects to this deposit being called the Lower Greensand, and says:—"The Shanklin (or Lower Green) Sand, as I understand it, is the series of beds between the Weald Clay and the Gault. But these sands at Potton are between the Gault and the Oxford Clay; and, so far as I remember, the only fossil previously recorded from the beds in this district is *Ammonites biplex*, mentioned in my paper on the Cretaceous beds at Ely,—neither of which facts offers any presumptive evidence of the deposit being Shanklin Sands." Here is his statement in the paper he refers to:—"The lower part of the Shanklin Sands is a conglomerate of small rounded pebbles, which in the best place in the section is hardly more than four feet thick; and above this are some brown sands alternating irregularly with thin courses of clay with phosphatic nodules;

* Geological Magazine, vol. iii. p. 153.

† This circumstance explains Mr. Brodie's apparently erroneous assertion that "every organism in this phosphatic bed is evidently extraneous," which was perfectly true with regard to the fossils obtainable when he wrote.

and in places these deposits almost stand on end, through false bedding. They are seven feet thick, and unfossiliferous, a good deal resembling the beds below; but I cannot say they should not be classed with the Gault. A rolled fragment or two of *Ammonites biplex* is the only fossil I have found in the rock; so that it might be Portland Sands but that it is traced to Hunstanton, where fossils are more numerous." Mr. Seeley then proceeds to trace the bed to near Potton and Sandy. He evidently at the time he published the above (December 1865) considered the bed to be of the same age as I do, but has since altered his opinion. I shall again have occasion to refer to the second paragraph quoted above. I am not aware that *Neithea quinquecostata* has ever been found in the Kimmeridge Clay at Weymouth or elsewhere.

II. Mr. Seeley says, "The term conglomerate applied to this bed is calculated to mislead," and gives a definition of what he thinks a conglomerate ought to be. In the paragraph already quoted Mr. Seeley applied this term to the same beds! I wished to involve the idea he objects to, viz. the denudation of older beds.

III. I stated that, if Mr. Seeley's views be correct, the term Carstone is inapplicable to the bed. On the idea that the Carstone at Hunstanton represents the Gault and Lower Greensand, he forms his remarkable hypothesis of the Significance of the Sequence of Rocks*. He now restricts the term to the sands of Yorkshire, Lincolnshire, and Norfolk, between the Hunstanton Limestone and the Kimmeridge Clay, and says, "But though I abandon the term, I do not abandon the idea," which idea he proceeds to illustrate by a diagram, but does not attempt to prove it; therefore I will not discuss the merits of it.

IV. I appear to have misunderstood Mr. Seeley's remarkable expression "the truth is, the 'Sandy nodule bed,' as this bed in the Carstone may be called, reproduces earlier in time the conditions of the Cambridge Greensand." I am very sorry; but it may be due to the ambiguity of the sentence tending to mislead. But I am still of opinion that two deposits so different in every respect as the Cambridge Greensand and the sandy conglomerate bed at Potton and elsewhere cannot have been accumulated under similar conditions. Mr. Seeley by no means explains the discrepancies between the two formations indicated in my former paper†, nor does he bring forward a particle of evidence in support of his assumption that both were formed upon a long low shore.

V. Mr. Seeley ascribes to me the "notable discovery that by soaking six or seven parts of alumina in decomposing animal

* Geological Magazine, vol. ii. pp. 262–265.

† Ann. Nat. Hist. ser. 3. vol. xviii. p. 383.

and vegetable matter till they increase to 100, you will produce a nodule of *phosphate of lime*." In return I may congratulate him on having made a still more "notable discovery," namely, that clay consists of pure alumina, which is evidently implied in his interpretation of my statements. Mr. Seeley ought to be aware that clay consists not of alumina, but of a silicate of alumina; and also that clays like the Oxford and Kimmeridge contain various other substances. Again, what Mr. Seeley denominates "rolled concretions of tolerably pure phosphate of lime" do not, in the best average samples, contain more than 22·39 per cent. of phosphoric acid = 48·51 per cent. of tricalcic phosphate, supposing it all combined with calcium (see analyses given in Mr. Brodie's paper). I hope at some future period to demonstrate the origin of these nodules by chemical analysis. The indication of the comparatively small amount of pure alumina contained in clays may serve to a certain extent to remove Mr. Seeley's difficulty as to what "becomes of the clay;" and I may also remind him that, on his part, he has not told us whence the alumina undoubtedly contained in the nodules is derived. To Mr. Seeley's objection to the word "soaked" I can only reply that I used it to indicate my belief that the clay derived from the sea-cliffs, formed of older beds, encloses and is saturated with animal and vegetable matter.

VI. Mr. Seeley repeats, "with diffidence, on account of the state of the specimens," that he gathered no extraneous fossils from the bed. It is "on account of the state of the specimens" that I regard them as derived from the denudation of older formations. The condition of the bones and teeth of reptiles and fishes shows that they have been rolled, and, moreover, rolled after fossilization.

VII. & VIII. Mr. Seeley complains that I did not take the trouble to get the phosphatic casts of the shells named; but he cautiously omits to give a list of those which he has determined to be Portland species; he also omits a list of the ferruginous shells. I gave a list of all I had obtained, when my paper was published, that were in a condition sufficiently perfect for determination.

IX. I am flattered by Mr. Seeley's remark that my list of Mollusca has "some approach to correctness." I am sorry that he does not add the "some few others" to his remarkable statement about the species of *Terebratulæ*. With regard to the fossil I have named *Ostrea macroptera*, he makes the following curious statement:—"Although this is the name used by me for this fossil, as a variety of the *O. frons* of Parkinson, it is a form limited, so far as I know, to the Portland Rock—very unlike Sowerby's typical *O. macroptera*." Why does Mr. Seeley

call this fossil by a name which he knows to be the wrong one? On referring to Prof. Morris's catalogue, I find that *O. macroptera* occurs in the Gault of Oxfordshire, in the Lower Greensand of Atherfield, and in the Greensand of Farringdon, where I found specimens during a recent visit. Mr. Seeley next states that he has seen no such shells as *Exogyra conica* &c., adding, "though I have long had other species of those genera in the Woodwardian Museum." He ought to have given a list of the specimens, which I presume, from his statement, have been presented by him to the University Collection.

X. With regard to this paragraph I can only say that, in my paper read before the British Association, I distinctly mentioned that fishes from the Kimmeridge Clay at Ely, specifically identical with those from Potton, were exhibited in the Woodwardian Museum, and that I think the rolled condition of the Potton specimens is a sufficient "reason for thinking them other than tenants of the sea of the time." I must confess that I am at a loss to understand the purpose of Mr. Seeley's reference to the existence of named specimens of these fishes in the University Museum, unless he considers that no one has a right to consult a public museum without acknowledging each occasion on which he may have derived information from it. As regards the specimens referred to in my paper, I had many of them in my possession and had determined them before any fossils from Potton were exhibited or, so far as I know, contained in the Woodwardian Museum.

XI. I will not be behind Mr. Seeley in confessing what I dare not call the *only* mistake in my paper. There occur in this bed rolled fragments of a rock composed almost entirely of shells; the specimens found were very much decomposed, and presented precisely the aspect of fragments of the *Cyrena*-bed. Since then, more boulders of this rock have been found, in a better state of preservation. On breaking these, I also have found specimens of *Cardium*; therefore I will admit that the specimens I mentioned in my paper probably contain the same shells. But I think that there is sufficient evidence of the denudation of the Wealden in the occurrence of the rolled bones of *Iguanodon* &c., and in the rolled fruits and wood. The wood exists in two different states of mineralization, as I remarked in my paper. Mr. Seeley states that he has shown in his paper "that the material of the deposit came from the east." I suppose he refers to one of his unpublished papers.

XII. The species described by me as *Sphæra Sedgwickii*, if not a *Sphæra*, is probably the type of a new genus; if, however, it should hereafter be proved to be a *Cyprina*, I have no doubt that it will be found to differ considerably from *C. angulata*, Sow.,

of which species Mr. Seeley says it is only a variety. *Pholas Dallasii* (mihi) appears to me to be nearly allied to D'Orbigny's *P. Cornueliana*; and both will, of course, take their place in the subgenus *Pholadidea*, as indicated by Mr. Seeley.

Finally, Mr. Seeley says: "The age of the beds is a difficult problem, and not one that can be solved by an appeal to fossils, or mineral character, or superposition." Unfortunately, Mr. Seeley does not inform us how the problem is to be solved, unless he wishes us to receive his hypotheses without requiring any proof. If I am honoured by a reply to my remarks, I may remind Mr. Seeley that, although the opinion of an eminent geologist must have great weight, yet it is by no means weakened by an appeal to facts, and that it is hardly fair to adduce in support of his arguments results said to be detailed in a book still unpublished, or in papers which have not yet appeared in print *.

XVIII.—*Note on the Species of the Genus* Tribonyx. By P. L. Sclater, M.A., Ph.D., F.R.S., Secretary to the Zoological Society of London.

In endeavouring to ascertain the correct scientific name of a fine specimen of a Ralloid bird of the genus *Tribonyx*, from Western Australia, which has lately been added to the Society's Collection, I have discovered that there seems to have been some little confusion between two of the species of this genus, which I take the opportunity of setting right.

Upon turning to Mr. Gould's 'Birds of Australia,' to which one naturally refers for the determination of an Australian bird, it is at once apparent that the Society's specimen is not the bird figured there as *Tribonyx Mortieri*, being distinguishable by its larger size and the distinct white stripes on the wings, although otherwise much resembling it. But, in his original description of *Tribonyx Mortieri*, Du Bus most clearly describes these

* Several examples of this citation of unpublished materials occur in Mr. Seeley's paper. I may refer more particularly to that which, as he says, was read on May 27th, 1867, before the Cambridge Philosophical Society, on a deposit near Upware. I was present on that occasion, and heard Mr. Seeley's remarks, with many of which, however, I could not concur, as I stated at the time. Mr. Seeley's so-called paper consisted apparently of an extempore exposition of his views. No list of fossils was given by him; and the whole paper was quite unworthy of an attempt to revolutionize the geological classification of the Upper Jurassic and Lower Cretaceous beds, in support of which it is cited in the last Number of the 'Annals.' I had already communicated (May 7th, 1867) a short paper on the Upware deposit to the Yorkshire Philosophical Society: this is printed in the 'Geological Magazine' for July.